II. THE CONCEPT OF FORCE

By J. J. C. SMART

MR. FOULKES[1] says that my "linguistic discussion"[2] misses the whole point of Hertz's work. And so, of course, it does. I was concerned with a logical issue, the question of the definability of "force", and discussed Hertz's work only in so far as it was affected by this issue. His system does not really supersede the Newtonian one (who would calculate the position of Jupiter using Hertzian mechanics?) but it has considerable theoretical importance which remains even if we are still convinced, as I am, of the logical respectability of Newtonian mechanics. Hertz's conception of a straightest path and many of his detailed mathematical techniques foreshadow in a remarkable way much of the general theory of relativity. In so far as these things are the most important things in Hertz's book, of course what I say misses the whole point of Hertz's work. Nevertheless you can do a good deed from a bad motive, and I thought that the motive, the desire to do without the Newtonian concept of force because of its lack of clarity, is of logical interest and worth discussing. Let us return to the question of whether the Newtonian concept of force is unclear.

I do not deny that the older writers on mechanics, including Newton himself (though not so often as people usually think), were unclear. The talk about *vis insita* and *vis impressa* referred to by Mr. Foulkes is a case in point. But that does not mean that the Newtonian system is *essentially* unclear, that it cannot be presented in an unobjectionable form. I referred to the paper on "Newton's Theory of Kinetics" by W. H. Macaulay,[3] which is a good example of

[1] *A.J.P.*, December, 1951.
[2] *A.J.P.*, May, 1951.
[3] *Bulletin of the American Mathematical Society*, Vol. III, 1896-7.

clear thinking about the Newtonian system. Of course Hertz's system may be in some respects *simpler* than Newton's. Mr. Foulkes refers to "the distinction between static and dynamic forces and the artificial connection which . . . has to be made between them".[4] It is to be remembered, however, that simplicity at one point is dearly bought by complications at others. Would Mr. Foulkes like to predict the motions of the planets using Hertzian methods? (In such cases astronomers use Hamilton's equations, and these can be deduced from Newton's laws.)

Mr. Foulkes says that Newton presents "force as a quality instead of recognising it as a relation". I am not sure what the evidence for this is, and in any case this is not the place to discuss the Andersonian panacea for all philosophical troubles ("don't you realise that what you are treating as a quality is really a relation?"). Let me say merely that it is not very clear to me what would be meant by someone who said *either* that force is a quality *or* that force is a relation. Is it either? Certainly it is correct to say that there is a force between two things, and this is analogous to talking of a relation between things. Instead of saying "the attraction between the particles A and B is F" we might say "the relation between A and B is an attractive force F". But we also say "force *on*". ("There is a force F *on* this particle.") We never say "relation on". We say (perhaps more or less misleadingly) that forces act on things. We never say that relations (nor qualities either) act on things. I will concede, however, that force is *more like* a relation than a quality. Even if I agree that it *is* a relation I am not conceding very much. The rules of the game Foulkes and I play are not very clear. Just what *are* the rules for conferring the honorific title "relation" on a concept? I prefer not to look at the concept of force through

[4] I think that Mr. Foulkes is misleading when he says "the Newtonian theory needs over and above the laws of motion the introduction of d'Alembert's principle", for d'Alembert's principle can be deduced from Newton's laws. But even if it were needed *in addition* this would only go to show that the Newtonian system is in this respect not as *simple* as Hertz's, say, not that it is *unclear*.

the spectacles of a two, three, or four category logic, but to try to see it as it is. Even if I do concede to Mr. Foulkes, however, that it is a relation, does this affect my question as to whether it is appropriate to ask for a *definition* of it?

On p. 177 Mr. Foulkes says "As far as the Newtonian theory is concerned they [the Newtonian laws] are empirical laws and not a definition of force". This is simply to deny, flatly and without giving any reasons at all, my original contention that they are neither quite empirical nor quite a definition but "both and neither". The question "are the Newtonian laws empirical or a definition of force?" is in my view not a proper question. It is like the question "have you stopped beating your wife?" The distinction between *theoretical* and *experimental* laws is unfortunately neglected by the logic books, but it is a vital one here. As examples of theoretical laws are Newton's laws of motion, Maxwell's electro-magnetic equations, Schrödinger's wave equations. As examples of experimental laws are Boyle's law, the law of expansion of metals, Snell's law for the refraction of light. Theoretical laws have meaning only as part of a theoretical system, and can only be verified *via* that system. Experimental laws can be verified purely experimentally; their variables all stand for measurable quantities, and they can be known antecedently to all theory. (Thus Boyle's law was known long before any theory was thought of which would explain it; Snell's law was known and verified long before the wave theory of light was ever thought of.) Mr. Foulkes suggests that the question "are the laws of motion analytic or synthetic?" is misleading in that it suggests that there could be analytic laws at all. I agree that a purely analytic proposition could not be a law of nature. Furthermore, an *experimental* law is indeed, as near as makes no difference, completely *synthetic*. It is important to notice, however, that a *completely synthetic* proposition could not be a *theoretical* law.

My objection to the analytic-synthetic and definable-indefinable dichotomies rests on the fact that language, except

within pure mathematics, and sometimes perhaps not even then, is not a game played according to a definite set of hard and fast rules. I tried to bring out how this is so in dynamics by considering the concept of force, and it is so in other parts of physics. Let us look again at the concept of "electron", which Mr. Foulkes also considers. I wish to take up his assertion " 'electron' is definable in the linguistic sense, for instance as 'the carrier of the least electric charge'. This form of words could be substituted wherever the word 'electron' occurs in a textbook on physics." Now could it? (Except in the trivial sense that we might *decide* to use "the carrier of the least electric charge" as a synonym for "electron" and without considering the meanings of its constituent words. Compare how "atom" has lost its connotation of being the ultimately indivisible particle.) Suppose you were teaching physics and a person had not attained the concept of electron. One would have to go through a long process; as I said in the earlier article, one would have to talk about the Wilson cloud chamber, about cathode rays, about Millikan's oil drop experiments, about the Bohr theory of the atom, about wave mechanics. One's pupil would come to learn to use the word "electron" more and more intelligently. Does any teacher of physics (such as Mr. Foulkes) *really* think that there is a magic short cut, just producing a formula like "carrier of the least electric charge"? Is it at all like the case of a teacher of anatomy, who in order to teach the meaning of the short-hand word "femur" just has to say "the femur is the thigh bone"? To go back to Mr. Foulkes' definition. The meaning of the expression "carrier of the least electric charge" seems to me to be *more* unclear than that of "electron". How can a person who does not know what an electron is know what it is that carries a charge? What *is* it to "carry" a charge? Putting aside these difficulties, is an electron that which carries the least charge? If we took the definition seriously we should have to say that neutrons are electrons, for they certainly carry the least charge, i.e. no charge at all. Again, what about

the difference between an electron and a positron and a proton? Suppose again that a revolutionary discovery is made in physics whereby a particle of less charge than that of an electron is discovered? Should we call this an "electron" and cease to call what are now called "electrons" by that name? A *decision* would have to be made (and this is important), though I have no doubt that in this case the decision would be to continue to call the things we now call electrons "electrons", even though they *didn't* carry the least charge. The point is not that we can pick holes in Mr. Foulkes' definition, for I am sure that he only meant it to be illustrative. My point is that I am confident that I could pick holes in *any* definition.[5]

It is a mistake to think of physics as though it were a completely formalised system like a logical calculus. Even if you formalised a physical theory (and this would fossilise it) you could never formalise the application of it. The steps from theory to experimental fact and *vice-versa* are even more important than the steps from one part of a theory to another. It is here that "judgment" enters in. Thus in Millikan's oil drop experiment the observed rates of fall of the drops might be such that they could be taken as falsifying the theory of discrete electronic charges, but these results would be explained by the experimenter as due to some such cause as the evaporation of the drops. No definite set of strict rules can be produced to tell the experimenter what inference to make in such cases—there is no knowing what range of experience he will have to draw upon, and whether an anomaly is one which is to be explained or explained away is a matter which must be left to the flair and judgment of the experimenter. Now if no strict rules can be laid down in advance to tell the physicist what deduction to make this means that no strict definitions of the physicist's terms can be given. For definitions and implications are related.

[5] It is instructive to recall the abortive attempts of philosophers to give a satisfactory definition of concepts like that of Time. See Waismann ("Analytic-Synthetic: II"), *Analysis*, Vol. 11, No. 2, 1950, p. 26.

I thoroughly agree with Mr. Foulkes that "the question of how certain linguistic usages gain prominence is not itself a linguistic question. Without attending to this latter non-linguistic question it is impossible to see the point of even a linguistic confusion." We must discuss language in the context of the situations within which and the purposes for which it is used. That is why to talk of philosophers doing "linguistic analysis" is so misleading. I also agree that it may well be that in learning physics people learn not only truths and insights, but also current confusions. A person who learns ordinary practical language cannot learn confusions; indeed I cannot imagine what it could be for simple practical language to be confused. A person who learns language in order to do such things as buy groceries is not likely to learn to speak in a confused way. If he asks for butter and gets butter how can what either he or the grocer says be confused? His language serves its purpose, and what more can language or any other human art do? On the other hand, in learning a sophisticated sort of talk like physics we may well learn to talk in a confused way. As Mr. Foulkes says, "it involves also the taking over of established traditions and along with them their attendant prejudices and confusions". I have nowhere shown that there are *no* confusions in Newtonian mechanics. What I have tried to show is that *one* confusion, the one which finds its expression in Hertz's complaint about the concept of force, is not to be found in Newtonian dynamics. It is thought to be there only because we persist in looking at the concepts of Newtonian dynamics through the spectacles of an inadequate logic. This inadequate logic forces us to ask the inappropriate questions "analytic or synthetic?" and "definable or indefinable?". Hertz had exceptional merit not only as a mathematician but as a philosopher. For though I think that he was mistaken in thinking that there is a confusion in the Newtonian concept of force, he did have an insight into the nature of philosophical perplexity, which was far in advance of his time, when he said: "our confused wish finds expression in the confused

question as to the nature of force and electricity. But the answer we want is not really an answer to this question. It is not by finding out more and fresh relations and connections that it can be answered; but by removing the contradictions between those already known, and thus perhaps by reducing their number. When these painful contradictions have been removed, the question as to the nature of force will not have been answered; but our minds, no longer vexed, will cease to ask illegitimate questions." [6] What I find surprising is that Mr. Foulkes seems to ignore the possibility that a question can be illegitimate or an indicative sentence anything other than true or false. Why *should* "is force definable or indefinable?" be a proper question? Simply because traditional logic bids us pose it?

[6] *The Principles of Mechanics*, translated by D. E. Jones and J. T. Walley, pp. 7-8.